

C. LINDEGREN
(FROM KEY WEST, FLA.
Postmarked Jan 14 '75.

Dear David, It's a long time ago and I'm not perfectly clear in all points. When I came to CalTech in 28 Morgan was himself dissenting Neurospora. But his procedures resulted in contamination with ~~soil~~ Penicillium. It stands to reason, and I believe I remember, that he was using *Setosphaeria* because these were the cultures that Dodge had published on. I did some work with the culture that Morgan was using, determining the temperatures for germination. I worked on a micromanipulator and the first things that I did involved hybrids between (I believe) *Setosphaeria* and *Tetraspora*. The results were impossible for me to interpret at that time. Before that I had done some work on seg of +/− in Tetra. But I can't remember where I got the cultures. I spent the summer of '30 in N.Y. with Dodge. He always wanted more work on *Tetraspora* and was disappointed that I couldn't see it as useful.

I must have got the Neurospora cultures from him that summer in N.Y.

The story of how Dodge got his cultures came to me from Thorn. There was working in the imperfect stage at the USDA. He established a race, showing how well it

was annoyed without a say real please. Shear
(his boss) saw prettiness in his cuttings
and turned them over to one of the women
in the dept. She was too slow for him
and he gave them to Dodge. When Shear &
Dodge published the several cycles, There was
furious. He told me he never would use the
word "Neurospora".

Crusoe just seemed to grow better than
sitopistis. I stayed with it except for one
division mentioned in my paper on Biseptal
Self-Strains Heterokaryons. This time Dodge
wanted to have some fun confusing me.
He sent me a culture & asked me to
tell him if it was his A or B, according to
my + or - O I didn't want to use
A & B because this procedure implied
2 loci. He couldn't understand my
reluctance. He refused to accept
the crossing or explanation until six
years until the summer meeting of 1939,

Regards

Carl